seen this variety in the field, its normal habitat lying considerably north of this locality. It would, however, be dangerous to base any conclusions upon an isolated case of this kind.

From the foregoing facts it seems probable that, in the case of those species which are amenable to the influences of climate, the stimulus necessary to induce seasonal change would consist in a combination of either moisture and heat or dryness and cold, and not in either of these factors exclusively.

Two years ago I made a few experiments in applying moist heat to the pupæ of several species of *Teracolus*. Unfortunately all my notes on the subject have been lost, but, so far as I can recollect, the results were almost entirely negative, which I then attributed to insufficient heat. The resulting specimens were, however, sent to the Oxford University Museum with full data, and such of the insects as are worth preserving from the present experiments are being sent to the same institution.

Salisbury, Mashonaland,
August 1901.

LI.—*A Revision of the Genera of the ARANEÆ or Spiders with reference to their Type Species.* By F. O. PICKARD
CAMBRIDGE, B.A.

IN the 'Annals & Magazine of Natural History' for January 1901 I have discussed the question of the determination of type species for various genera and have briefly detailed the main features of that method of selecting type species amongst *heterotypical* genera which is usually known as "*elimination*" or "*exhaustion*."

My remarks have called forth prompt criticism from Dr. Dahl, in a paper published in the 'Archiv für Naturgeschichte,' Jahrg. 1901, under the title "Die internationalen Nomenclaturregeln und ihre Anwendung auf die ältesten Spinnengattungen."

In this paper the author questions not only the application of the principles laid down in relation to the special cases dealt with, and the value of the methods themselves, but he even goes so far as to doubt whether there is any necessity for the selection of a type species at all.

Now, before we enter the field of discussion as to the methods and rules whereby a type species is to be recognized,

we must be quite certain that we have made up our minds first of all as to whether, in the interests of practical systematic zoology, a type species is to be selected or not.

It is still an open question for some. The general morphologist, for instance, who is under the impression that a thorough knowledge of the anatomy of the common stag-beetle sufficiently warrants him in speaking authoritatively on leading questions involving the whole family of the Coleoptera, may perhaps not appreciate the necessity; but the systematist who has to deal with countless generic divisions, subdivisions, and names, and to determine what characters those names are to connote and what species to denote, who is familiar with the hopeless chaos arising because no two authors agree on those points, and who wishes to see some definite settlement of them, this man will not hesitate as to the course to be adopted. And although Dr. Dahl gaily enters the lists in this criticism of methods, he has evidently not made up his own mind as to whether it is necessary to select a type species or not; for he says, "*I think we are acting in perfect accordance with the spirit of the rules of nomenclature if we . . . in certain circumstances even acknowledge several typical species for one genus.*"

Well, Dr. Dahl is of course perfectly entitled to his own opinion on the matter; but one wonders how came *he* into this galley at all? If he does not acknowledge the necessity of selecting a type species for a genus, he has evidently had no great practical acquaintance with modern systematic zoology—not sufficient, at any rate, to justify him in a wholesale criticism of the comparative merits of the various methods of selecting types.

If Dr. Dahl's attitude be really, as he suggests, in accordance with the spirit of the "Rules of Nomenclature"—a spirit which allows him to select a single type in one case and three or four in another,—well, then, so much the worse for those rules, and the sooner they adapt themselves to the needs of systematic zoology the better will it be for those who are practically engaged therein.

In criticizing the methods followed in my paper Dr. Dahl declares that "*the writer follows his own rules of nomenclature.*" I must say I am surprised that Dr. Dahl should be so very ill-acquainted with the whole problem of the selection of types as to credit me with the invention of methods which have been well known for years to the majority of the ablest systematists of the day in most branches of zoology.

He, moreover, states that those methods are contrary to the spirit and letter of the International Rules of Nomenclature.

As a matter of fact, the adoption of the method of "*elimination*" is simply the logical application of the very rule which Dr. Dahl himself quotes as follows :—" *The first publication in which a genus is subdivided, whether justifiably or unjustifiably, whether in a conscious or an unconscious manner, must, where no typical form was named, decide what portion of the original genus is to retain the original name.*"

If the first publication leaves more than one species in the genus, then the next publication which again subdivides the residue will decide what portion of the original genus is to retain the original name, and so on until one species only is left, and this remains as the type. And if not, why not? And if so, this is just simply the process of "*elimination*" or "*exhaustion.*"

If the International Rules allow an author to limit a genus to ten species, on what logical grounds will they forbid him from limiting it to ONE species? And if an author definitely selects (or cites) a single species as the type, he is simply, "*justifiably or unjustifiably, consciously or unconsciously,*" deciding what portion of the original genus (a single species in this case) shall retain the original name. The process is not precisely similar to that of elimination, but for all practical purposes it is the same.

Elimination pure and simple.

Now elimination pure and simple proves in its practical application almost invariably to land us in an absurdity. In this way: the species which authors withdraw are usually those that are best known, with characters salient and well described, leaving in those less well known—with this result, that the last species left in is one which is not known, is badly described, and is never likely to be identified with any certainty; and this miserable phantom is left us as the type of the genus. In its simplest and purest form the process often ends in a futility.

But by recognizing the *definite selection or citation* of a type species systematists may overcome this difficulty, and by a process not inconsistent with the Rules of International Nomenclature. Because, it will be noted, most authors in citing a type species have selected as typical those that are well known to them, and hence the genus will be represented by a species whose characters can be always ascertained; so that for "*practical purposes,*" and we are now concerned with none other, the process or method which most of all serves in bringing about a reasonable and practical result is one of

elimination tempered by definite selection or citation—the one object held in view being the narrowing, as early as possible in the history of a genus, the area within which the type species must be sought for, always presuming that this shall be one of those species originally included when the genus was first founded and the generic name bestowed.

The Problem before us.

We must, then, in connexion with this problem make up our minds on the following points:—

A. As to the date from which our nomenclature is to begin.

B. As to the necessity for fixing upon a single species as the type of each genus.

C. As to the method of fixing upon a single species as the type in the case of heterotypical genera.

With regard to the first point: in my paper referred to above I had not excluded the names published by Clerck in 1757, because I considered that they answered all the requirements of the binomial system. It is of course quite absurd to exclude Clerck merely because he wrote one year earlier than the date arbitrarily fixed upon as the point whence our nomenclature should start—the tenth edition of Linnæus, 1758.

None the less, however, I agree with Dahl that if we are to accept the tenth edition as the starting-point, then Clerck's names must go; for this is just a case in point where we shall never get agreement unless we adhere absolutely to this first rule. There is no question of right or wrong in the matter, nor even of common sense—rather the contrary; it is simply a question of convenience, a step taken for "*purely practical purposes*," as Dr. Dahl himself suggests, in order to avoid disputation and to bring arachnological nomenclature into line with that of other branches of zoology.

B.—It is not necessary to enlarge upon the necessity of fixing upon a single species as the type. This, again, is a purely practical question, for theoretically there is no type of a genus. And this practical type is a species, in the case of arachnology consisting of two sexes, round which others group themselves, their characters fining away all round the circumference, where they pass into other adjacent groups. Practically we have to decide upon the species which is to be regarded as the centre of this group.

Dr. Dahl, in pointing out that the International Rules make no mention of the right of an author to select a type, adds that "*they speak only of the original type, and in this they do*

well." He admits, then, that the *original type* (not several types) is to be sought for under the International Rules; but that is precisely the problem before us, to settle what is to be considered as this original type.

Dahl would have us analyze and weigh every word of an author, in order that we may glean, if possible, some inkling as to what species he would seem to have had more especially in his mind when he founded a genus, so that we might select that species which appeared to embody more fully the characters of the genus according to the author's original idea.

Theoretically this course appears reasonable, for practical purposes it is impossible. How many arachnologists would agree, first, as to what the original idea of the genus was, and second, as to which species fulfilled all the requirements of that idea? In this way we launch ourselves at once into that labyrinth of hair-splitting arguments out of which there is afterwards no escape for the disputants and no practical way out for a type. It is exactly this fatal quagmire that we have to avoid if we are ever to come to any agreement at all. There are a quite sufficient number of "*hard cases*" to exercise our analytical faculties over in minor details without rendering the settlement of every case impossible at the outset. But on what practical grounds can Dahl, ostensibly seeking for Latreille's original type of *Salticus*, for instance, refuse to recognize the species which the author himself a few years later cited as "*type*"? Is it likely, without necessarily converting the word type into a fetish, that Latreille would have cited the species which he did *not* regard as fairly embodying his idea of the genus? Is it not more reasonable to suppose the contrary to be the case?

In fact Dahl himself says:—"Since later authors ought as closely as possible to follow the original author, the species specially named by the latter must, by them also, be the first to be considered." Precisely so! And for practical purposes the adoption of the type cited saves us a great deal of unnecessary trouble. Why, then, ignore Latreille's citations in 1810?

But of course every one has a right to his own opinion, and if Dr. Dahl does not agree that it is necessary to select a type species at all, I fully respect his opinion. But it is of no use discussing with him as to the methods to be adopted in the process, when he does not even agree that the process itself is necessary.

C.—On the method of fixing upon a single species as the type in the case of heterotypical genera. There are four methods:—1. Elimination or exhaustion; 2. Restriction;

3. Citation or selection of a type; 4. Selection on account of the identity of generic or specific names. We have to consider under what circumstances a genus can be regarded as legitimately restricted either by elimination, by restriction, or by the citation of a type.

(1) To take the case of *Pure Elimination* first. When a genus is subdivided or broken up by an author and one or more new genera formed out of the original species, leaving one or more species under the original name, this is legitimate elimination. But there is no elimination when the original genus is merely cited and one or more of the original species cited under it. There must be at least *one* new genus founded out of the original species at the time when we claim subdivision or elimination to have taken place.

The reasons for this will be obvious. In the first place, it will confine us to works that are definitely systematic, and not merely *lists*; and, secondly, we shall exclude the works of those who were merely compiling a popular treatise, without any intention of advancing the systematic knowledge of the group—"Ignoramuses," as Dr. Dahl prefers to term them. By adopting this course, too, we shall save ourselves the trouble of undertaking half a century of research in the British Museum, examining every newspaper, periodical, magazine, and popular treatise, lest perchance some author has unwittingly restricted a genus by some unconscious elimination of certain species originally included in it.

(2) Next, to take the case of "*restriction*." When an author definitely states that he wishes it to be understood that a genus be restricted to such and such species, or cites certain species and adds the formula "*sensu restricto*," the conditions under which this restriction takes place are in themselves a sufficient guarantee that the work is *systematic* and not that of an "*Ignoramus*."

(3) With regard to the third method, namely that of the selection or citation of a type, we have to be equally explicit, for purely practical purposes, as to the circumstances under which such action can be regarded as valid. It is not a question as to what early authors meant by the terms "*type*" or "*example*," or what they did not mean; nor is it a question of what we think they meant. They themselves knew, in all probability, exactly what they meant by these terms, though it is quite clear that we ourselves cannot say for certain what they did or did not mean.

But the practical question for us is, what meaning are *we* going to attach to these terms—*typ*, *typus*, *type*, and *exemplum*, *exemple*, *example*? For we must be quite clear in

our own minds on the matter, just as we were on the question of the tenth edition of Linnæus. It is not a question of right or wrong, nor of common sense; it is purely a question as to what course is for practical purposes the simplest and least likely to lead to disputation.

Personally, and I have no wish to lay down any law for any one, I am inclined to believe that the line should be drawn at "*example*." That we should assume that when an author cites a single species with the additional designation of *typ*, *typus*, or *type*, he was definitely citing what he regarded as the species fairly embodying the characters of the genus, provided, of course, that it was originally included in the genus.

Thorell, as long ago as 1870, recognized the significance of this term and himself selected "*types*"; Simon has also taken the same line; and the only point one would insist upon is that we shall not allow ourselves to open up disputation as to what an author did or did not mean when he used the term. The point is that he has done so in printed fact, and for our present practical purpose that is sufficient.

I would not admit "*example*," because it is more than probable that this word has frequently been used by writers who had no idea of definite systematic study; and if we did so we should instantly open the door for the entrance into the field of enquiry of all those popular and trivial works referred to above, which are well excluded by confining ourselves to those selections cited under the word "*type*."

One does not for a moment suggest that this course has any great underlying principle which would commend it, but that it is the simplest for those whose only object in view is to narrow down as early as possible the area within which the type need be sought for—no unimportant consideration in cases where sixty or seventy species were originally included.

This course will, I have little doubt, be adopted by all authors, as it has been by very many already who have had abundant experience in actually dealing with the complex questions involved, and have ascertained on what lines absurdities can best be avoided and some definite result reached. Men whose knowledge of the subject is purely theoretical will probably not agree on *any* lines that may be suggested, for the simple reason that there is not one of the lines proposed against which one or more weighty objections cannot be urged. This we freely admit, and it is necessary then, if we are determined to arrive at some practical result, to choose that method against which fewest objections can be urged by those who have actually worked out results.

(4) The custom of regarding as the type of a genus that species whose specific name has been adopted as the name of the genus including it has long been followed by zoologists. Examples of such identity of generic and specific names will readily occur—e. g. *Lutra lutra*, *Scomber scomber*, *Astacus astacus*, *Avicularia avicularia*, &c.

The reason for such custom is clear. If a species be definitely referred to by the author in connexion with the formation of a genus, it is assumed that the author himself regarded this as the type. How then, it is argued, can an author refer more pointedly to a species than by adopting its specific name to denote his new genus? This species, then, is regarded as the type and has prior claim as such over any other left by subsequent process of elimination or citation, just as would be the case if the author himself had cited the type at the time of founding the genus.

But in any case the species must have been originally included in the genus, e. g. *Avicularia avicularia* (Linn.). The type, however, of *Tarentula*, Sundevall, cannot be *T. tarentula* (Fabr.), because this species was not originally included amongst those referred to the genus. Fortunately very few cases of such identity of name occur, so that complications are not likely to arise.

The oldest Species as the Type.

There appear to me to be no reasonable grounds for supporting the contention that the oldest species ought to be regarded as the type in heterotypical genera where no type has been previously cited, definitely indicated, or left in by exhaustion.

No one can confidently assert that authors always had the *oldest* species in their minds when they diagnosed their new genera, any more than that they had the *first in order of publication* in their minds. One might equally well assert that the *middle* species of the series would be more especially in the author's mind, and that he was grouping the others round this central typical form. There can be no grounds for either of those assumptions, and no two authors would nowadays agree on any one of them.

It seems to me better to leave the selection in such cases entirely to the option of the selector.

There are several other points also about which we must make up our minds, to which I now briefly refer.

A Generic "Nomen nudum."

Much diversity of opinion appears to exist also on this subject, but, none the less, if we are to make any attempt to settle the type species of a genus we shall have to make up our minds under what circumstances a generic name is valid and when a *nomen nudum*.

There are two sets of circumstances under which a generic name might conceivably be regarded as null and void, and one in which it is not easy to decide:—

(1) When a generic name is proposed and published without either generic diagnosis or any species cited by name or cited by reference to some group name or sectional name, involving certain species under them, previously published.

In this case, since there are no means whatever of ascertaining the characters on which the genus is founded, the generic name is null and void.

(2) When a generic name is published with a generic diagnosis, but without any species being cited either by name or by reference.

In this case, again, the type species cannot be fixed, and though the generic characters are given, unless a definite species be cited or referred to, it is difficult to understand how the genus can be valid, seeing that theoretically a genus represents a group of species, and must, for practical purposes at any rate, be represented by at least *one* species definitely cited or referred to. Dr. Dahl would, if further proceedings interest him in any way, agree to this, for in his analysis of the spirit of the Rules of Nomenclature, in his search for the principles on which they are based, he has discovered—PRINCIPLE IV. "*Typical species, in the rules of nomenclature, take the place of the conception of genus*": with this result, that "*the purely formal basis is retained, and the fluctuating conception of genus avoided.*" Quite so! That is precisely what we are striving to point out. Our efforts are directed to secure this "*formal basis*," so as to avoid that "*fluctuating conception of a genus*" which is so exasperating to the modern student of the Araneæ.

(3) The next case is a doubtful one. When the species quoted or referred to actually contradict the generic diagnosis in one or more characters.

This is a difficult question, because at first sight it seems that if a species in any of its characters contradicts the diagnosis it cannot be the type of that genus. But supposing the character is not *essential*—e. g. the central anterior eyes are two and a half diameters apart, whereas the generic

diagnosis quotes them as two diameters apart, while the rest of the characters coincide with the diagnosis. Can one hold that this would exclude the species from being regarded as the type? Who is to settle what is the essential character of a genus and what is not? No two authors would agree. Personally, I should hold that since we are looking for a formal basis, the best one we can find will be one of the species originally referred to the genus, quite irrespective of whether the author described their characters correctly in his generic diagnosis or not. One would not, of course, if the selection of a type were left to one's self, deliberately select the species which was thus contradictory to the generic diagnosis; but otherwise one would have to treat the species as though there was no contradiction. That at least is my personal opinion. The fact is that it is not the generic diagnosis (the conception of the genus) which is all-important, but the species referred to the name; these are all-important, for from one or other of them the characters of the genus can always be ascertained, especially if one be selected as the type. Dr. Dahl will agree with this, being merely the practical application of his Principle IV., though his attitude in other respects is not altogether consistent with his adherence to the principle he himself claims to have discovered.

It follows, then, that there are two circumstances under which a generic name is clearly valid:—

(1) When accompanied by a diagnosis and with one or more species cited by name or clearly included by reference to some former publication.

(2) When not accompanied by a diagnosis, but with one or more species cited by name or included by reference.

Note.—The generic diagnosis is really included in the species. In the case of the Araneidea we should be obliged to exclude almost all of C. L. Koch's generic names if the presence of a generic diagnosis were held to be indispensable to an available generic name.

When a Type Species has been wrongly identified.

Another case giving rise to much difference of opinion runs as follows:—An author founds a new genus with a diagnosis, and cites as the *type* a single species already described. It is afterwards discovered that the specimen diagnosed is not the species the author supposed; it has been wrongly identified.

The question is, What *now* is the type of the new genus, the species cited by name or the specimen which has been wrongly identified?

To give a concrete case in illustration. The new genus

Melopæus, accompanied by a diagnosis, was based on a specimen in the Brit. Mus. Coll. identified by the author as *Selenocosmia albostrata*, which is cited as the type.

Now if *S. albostrata* has in this instance been wrongly identified, what is to be regarded as the type of *Melopæus*?

Some authors consider that the species cited by name must be the type, and the name *Melopæus* will become a synonym of *Selenocosmia*, or, at any rate, will follow the fortunes of *S. albostrata*.

Others regard this as absurd, a mere confounding the symbol with the thing signified, and hold that the specimen, though wrongly identified, is, and always must be, the type of *Melopæus*.

Now there are two possible circumstances—

(1) In which the specimen, wrongly identified, has never been described and has no name.

In this case *Melopæus* becomes a generic "*nomen nudum*," because, though the genus is diagnosed, it is based upon a non-existent species, and therefore there can be no *type species* of the genus.

(2) In which the example, wrongly identified, is found to have already been described under another name.

Then this species thus named is the type of *Melopæus*, and the name *Melopæus* will follow the fortunes of the type species.

There is no doubt that *Melopæus* must, for weal or woe, follow the fortunes of the specimen whence the diagnosis is drawn. If the latter be non-existent as a recognized species, *Melopæus* falls; if the specimen diagnosed be referable to another genus under a recognized specific name, *Melopæus* becomes a synonym under that genus.

It is, however, of course open to the author to describe the example, using if he please the original generic name; but if another author has meanwhile described the species under another new generic name, then *Melopæus* becomes its synonym.

This at least, in my humble opinion, is the solution of the case in question.

Priority in Pagination.

Every one will agree with Dr. Dahl that there is no necessity to point out that the first pages of a work cannot be considered in reference to their date of publication as older than the later pages.

The choice of a name, specific or generic, which occurs on

an earlier page in preference to one printed on a later page, or a later line on the same page, can be defended simply on the grounds that it is an extremely useful course to adopt for "*practical purposes*." We have to select *one* name, why not select that which occurs first? It occurs first in the manuscript, it was first set up in type, if the question of priority be discussed; but whether or not makes no difference. It is a simple straightforward method of settling a difficulty. But some people do not like simple methods; they prefer to enter into labyrinthine arguments rather than settle the matter. Does it simplify the process of determining which specific name is to hold good out of three, each of which was applied to a variety of the same species, by asking, for instance, under which name the "*forma principalis*" is described? Who will agree as to the "*forma principalis*," when the form which is a variety in one country may be the principal one in another?

Of course, if it be not agreed that we want to reduce these methods to their simplest possible terms, there is no need to waste time discussing the point either on this or any other question.

In conclusion, I regret that pressure of other work has prevented me replying to Dr. Dahl's paper before, and even now hinders me from dealing with the question of the types of the Latreillean genera just at present. This, however, I hope to do shortly, and also from time to time with the others on the lines briefly indicated above. But I should like it to be distinctly understood that I have no wish or intention of laying down the law for any one else on any point; but having come to the conclusion that, if the confusion in the nomenclature of the Arachnida is not to become worse confounded year by year, the types must be selected and some definite conclusion arrived at with regard to them. Practical acquaintance with the difficulty and the intricacy of the questions involved has suggested the remarks made above as to the methods to be followed. These are not my own methods, but have been followed, tentatively at any rate, by many authors for years. I may remark, however, that I have not given adherence to any of them without first applying the test of their practical application to the problem before us. And, finally, I would like to point out again to Dr. Dahl that, far from being contrary to the spirit of "*Die internationalen Nomenclaturregeln*," these methods are quite consistent with both letter and spirit as he himself has quoted and interpreted them respectively.

Pickard-Cambridge, F. O. 1901. "LI.—A Revision of the Genera of the ARANEÆ or Spiders with reference to their Type Species." *The Annals and magazine of natural history; zoology, botany, and geology* 8, 403–414.

<https://doi.org/10.1080/03745480109443338>.

View This Item Online: <https://www.biodiversitylibrary.org/item/94923>

DOI: <https://doi.org/10.1080/03745480109443338>

Permalink: <https://www.biodiversitylibrary.org/partpdf/68337>

Holding Institution

Harvard University, Museum of Comparative Zoology, Ernst Mayr Library

Sponsored by

Harvard University, Museum of Comparative Zoology, Ernst Mayr Library

Copyright & Reuse

Copyright Status: Public domain. The BHL considers that this work is no longer under copyright protection.

This document was created from content at the **Biodiversity Heritage Library**, the world's largest open access digital library for biodiversity literature and archives. Visit BHL at <https://www.biodiversitylibrary.org>.